adoption of a uniform twist of rifling, now slow-burning powder is used. The increasing twist of rifling was very probably effective in saving the gun when quickly-exploding powder was employed. But the importance of an increasing twist of rifling decreases as the action of the powder gas is rendered more nearly uniform. If the pressure driving the projectile throughout could be made perfectly uniform, then a uniform twist would exert a constant force to produce rotation.

Mr. Longridge says :—"So long ago as 17th March, 1860, the then Secretary of State for War, in his speech on the Army Estimates, said that 'these experiments proved that they had been wrong in using powder of so quick a detonating nature for artillery practice, and especially for rifled cannon, which required a weaker and slower powder than in the other cases" (p. 113). And twenty-four years later, March 20, 1884, the Secretary to the Admiralty said: "The old breech-loader had been found to be of no more use than a muzzle-loader, and the Government had adopted a gun twice as long as the old form of breech-loader." It is not very clear what all this means, but it is plain that vast sums of money will be required to provide long guns. Twenty years ago it might have been determined what effect every additional foot in length of a gun had in imparting increased initial velocity and increased steadiness to projectiles; but something more than the "rule of thumb" would be required to accomplish this.

England has of late come to acknowledge the value of technical training, and has shown a readiness to take a lesson from Continental nations. Is it not natural to suppose that some training of this kind might be found useful in settling the proportions of our guns, and in other matters of the same kind?

We think that Mr. Longridge has made out his case, and that his system deserves a fair trial in comparison with other promising systems. It has already been deemed worthy of a partial trial at Elswick, in France, and America. Experimental guns on different promising systems might in the first case be constructed of small calibre, and adapted to fire the service projectiles. If these proved satisfactory, then proceed to construct larger guns, and finally let that system survive which was found best fitted for its purpose. F. B.

OUR BOOK SHELF

A History of British Birds. By the late William Yarrell, V.P.L.S., F.Z.S. Fourth Edition. Revised to the end of the second volume by Alfred Newton, M.A., F.R.S., continued by Howard Saunders, F.L.S., F.Z.S. Parts XXI.-XXIV., January to July, 1884. (London: Van Voorst.)

ALTHOUGH, as we have said in a former notice of this work, it was a great pity that Prof. Newton could not be induced to complete his revised edition of Yarrell's well-known "History of British Birds,"—a subject in which he is before all other living naturalists at home, there can be no doubt that the task has fallen into good hands. Mr. Howard Saunders has not only completed a volume in about the same space of time that the former editor took to issue a single number, but has performed his work in a style to which, we think, little exception will be taken. The aim of Yarrell's "History of British Birds," we suppose, is to be sufficiently popular to be understood by all well-educated people, and at the same time to be thoroughly correct in scientific matters, so far as they are involved. As to Mr. Saunders' numerous remarks upon points of synonymy and distribution being entirely free from error, we should be very sorry to guarantee anything of the sort. But as we turn over the pages of his recent numbers, very few exceptional statements seem to present themselves, and most of these relate to what are to a certain extent matters of opinion.

Having finished his *Limicolæ* in Part XXI., Mr. Howard Saunders naturally proceeds to the Gaviæ, a part of his subject with which he is, as we all know, very familiar. An author who has worked out the Laridæ of the whole world in a thoroughly conscientious manner, and made this group his special study, is above all others qualified to prepare a special account of the "British" species. Of these Mr. Saunders recognises thirty as admissible into the list, though it is more than probable that this already large number will be still increased by the arrival in future years of stray individuals belonging to other species of this essentially wandering tribe of birds.

Congratulating our author on the accomplishment of the first volume of his portion of this excellent work, we may venture to express a hope that he will bring the long-delayed fourth edition of "Yarrell's Birds" to a speedy and satisfactory conclusion.

Bulletin of the United States National Museum. No. 19. "Nomenclator Zoologicus." By Samuel H. Scudder. 8vo. (Washington, 1882-1884.)

MR. SCUDDER'S "Nomenclator Zoologicus," which has been issued as No. 19 of the *Bulletins* of the United States National Museum, is not of a generally attractive nature, but will be of great use to working zoologists. It consists of two parts: the first of these, or "Supplemental List," contains the names of genera in zoology established previous to 1884, which are either not recorded or erroneously given in the previously issued Nomenclators of Agassiz and Marschall. To each generic name is added a reference to the work in which it is to be found. The second portion of Mr. Scudder's volume, or "Universal Index," contains an alphabetical index of all the names included in the "Supplemental List," together with those given in the Nomenclators of Agassiz and Marschall, and in the Indices of the Zoological Records. This second, most important part, contains about 80,000 entries, and, if correctly drawn up, as we have no reason to doubt is the case, will enable a naturalist who has recourse to it to determine at a glance whether any particular name has been already employed in zoology or not. All working naturalists will at once acknowledge the value of such an index as this, and will join us in thanking Mr. Scudder for having produced it. Could Mr. Scudder's index be magnified into a "Lexicon Zoologicum," giving the references to all the 80,000 terms in a single volume, a still more meritorious and useful work would be the result. Until this shall have been accomplished, it will still be necessary for a naturalist to refer to half a dozen or more different works in order to ascertain where any particular generic term has been employed in zoology.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Krakatão

I FORWARD a letter recently received from a former pupil, Dr. Stanley M. Rendall, which gives so graphic a description of the

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sea of pumice which resulted from the eruption of Krakatoa, that you may perhaps not be indisposed to print it in NATURE.

University of Edinburgh, July 17 WM. TURNER

71, Rue de Genève, Aix-les-Bains, June 24, 1884

DEAR PROFESSOR TURNER,—I have been so occupied since meeting you in Edinburgh that it has been impossible to send sooner notes of my trip to Java. The first intimation that I had of anything unusual having occurred was on our way out to Australia. Making our way north again after having done our "easting" in about 45° S., we were all much struck by the splendidly vivid sunsets, and by the distinct interval of time between the actual disappearance of the sun below the horizon and the appearance of the deep red or crimson glow. This phenomenon was more striking as we sailed north. On reaching Queensland we heard of the volcanic eruption in the Sunda Straits and received the explanation of the so-called "Krakatoa Sunsets." I left Newcastle, N.S.W., for Batavia in a steamer about November 10, 1883, *viâ* Cape Leeuwin, sailing up the west coast of Australia. We first came across distinct evidences of the eruption about 200 miles before we entered the Straits of Sunda, in small isolated pieces of pumice-stone, which became much more numerous and finally took the form of yellow patches constituted by morsels of pumice varying in size from a pea or even smaller up to a cocoa-nut, rarely larger. As we neared Krakatoa itself, these patches were certainly more numerous and larger in size, but still the actual amount of debris was small, much smaller than I had expected to find after the account I had heard from persons who had previously traversed the Straits. The yellow patches were few and far between, and composed chiefly of a coarse dust with here and there larger lumps amongst it. Krakatoa, formerly the most fertile of all the lovely isles which one passes, rose like a vast cinder, still smoking. Not a blade of grass or a leaf was to be seen, just a grayish seared-looking mass. A large portion of the island had disappeared, and I was told that over the sunken part 300-fathom soundings had been taken. Between the island and Batavia we passed a few more floating patches of pumice as above described. After about eight days at Batavia we steamed down the coast to different ports, Cheribon, Tegal, and Samarang, being on the whole about three weeks away from the time we left Batavia to our return there, which took place, so far as I can remember, on January 1. On nearing Batavia again we passed through large patches of pumice-stone, patches of several acres in extent, some of the lumps forming them being of large size, roughly speaking about as big as a cwt. sack of coals, and all sizes below that down to coarse dust. We anchored for the night just outside the port in clear water. Early in the morning one of the officers called me to look at an immense floe of pumice-stone that was bearing down upon the ship, and very soon we were entirely surrounded, and formed the centre of about, I should think, a square mile. Though covering a large surface, there was evidently no great depth of matter. One could pick it up by throwing a bucket or heavy pail into the mass, and a small steam launch easily made her way through it to our side. We left Batavia early in the morning, and passed through two or three such collections, all making their way in the same direction by the action of a current, as there was no wind, the sea being perfectly calm. When about thirty miles from Batavia, we met coming towards us an immense field, similar, except for its greater extent, to those already described. I could not tell you by any means exactly how large a surface it covered, but at one time could only just make out the edge we had entered it at with the naked eye, and could not see its termination in the opposite direction with the ship's glass, so that it was at least several miles in extent. Also the depth of the pumice-stone bed was very great, offering considerable resistance to the ship's progress, as shown by its diminished speed. An iron fire-bar thrown over the side rested on the surface of the mass, instead of sinking. Large trunks of trees were not floating in the water, but resting on the surface of the pumice. The passage of our vessel left a wake of only a few feet, which speedily closed in again, so that to see it at all I had to lean over the stern and look under it as it were. It seemed exactly as if we were steaming through dry land, the ship acting as a plough, turning up on each side of her a large mound of pumice, especially noticeable on looking over the bows. Our passage through this made no great noise—just a soft sort of crushing sound. The effect was very striking and queer. I only regret that I did not time our passage through this, the largest mass we met. We passed through one more but smaller field in the Straits of Sunda, and after that do not remember again meeting with any even small patches of pumice-stone. I thought it curious meeting such immense quantities of the debris in the same place where, a month or five weeks earlier, only a few scanty, isolated patches existed. It was not due to a new eruption, so must be accounted for by the currents massing together a large number of scattered patches; or perhaps a certain amount had first sunk, and then, later on, had risen to the surface. I hope these short notes may be what you want; if I can give you any more information, I shall be delighted to do so.

With kindest regards,
Believe me yours sincerely,
STANLEY M. RENDALL

The Laws of Volume and Specific Heat

THE former, known as the "law of Avogadro," implies that any given volume at the same temperature and pressure must contain the same number of molecules. It includes the law of Chasles, viz. equal expansibility for equal increments of heat; and the law of Boyle or Marriotte, that the volume of any gas must vary inversely as the pressure.

The other is that of Dulong and Pettit; and as the former necessitated equal volumes, so this latter implies constant heats for parallel conditions. But, finding that few elements approximated this law, it was an early device to double, treble, or quadruple the old atomic weights to secure a supposed uniformity; and thus the law found this expression, viz. that the specific heat of any solid element would prove to be a measure of its atomic quantity.

This, put in plausible fashion, will be the stock instruction of the superficial books for some time to come; but in the higher circles of chemical life it is being admitted more and more that a great change has come over the spirit of this dream. Departures from the normal 6·4 are no longer attributed to errors of observation, and that constant is replaced by a range of 5·5 to 6·9; while, to keep within this, M. Weber has proved that the doubled carbon equivalent must be tested at a range of temperature exceeding 1000° C. He has found that within the limits of –50° and 600° its heat value increases sevenfold! Well indeed may he say, "The idea that temperature can be overlooked must no longer be entertained;" also, "That the specific heats are not generally expressed by constant numbers; the physical condition of the elements influence their specific heats as much as their chemical nature."

These be great admissions from one of the highest authority, but they are as nothing compared with the new demands of physical chemistry. Mr. J. T. Sprague, an able and determined new chemist, has been the first in England to challenge attention to the recent researches of M. Berthelot, L. Troost, and others of the very highest chemical authority.

In a recent paper he admits that the new results "strike at the root of the most favourite chemical doctrines of the day, doctrines which are the foundation of the modern atomic weight and molecular theory, and consequently of the doctrines of atomicity, and the complicated molecular theories which have been based upon the supposed atomicity and specific bonds of different atoms."

The laws of Avogadro and Dulong and Pettit are offshoots of one principle, and one really implies the other. *If* true, it would follow that the atomic heat must be the same for all substances, or, if otherwise, the same quantity of heat would not produce equal expansions; also that the specific heats must be equal at all temperatures, or equal quantities of heat would act differently at different temperatures, or else it must vary equally for all gases, or they would expand unequally for equal quantities of heat.

Now it is a misfortune for these laws that none of these conditions subsist over wide areas. As a consequence of the two laws, an air thermometer should measure all temperatures by equal rates of expansion, and a given expansion should correspond to a fixed quantity of heat; such a thermometer should also read equally if filled with any other perfect gas.

In other words, these laws can only be true if the relation between the weight and volume of different gases be constant, and if the heat absorbed in producing a given change of volume is equal at all temperatures; that is, if the specific heat is constant.

These conditions are practically fulfilled by air, O, N, and H, between 0° and 200° C., so that the scale of temperature derived from the change of volume is the same as the scale derived from quantities of heat; but between 200° and 4500° there is a gradual growth of changed conditions which proves fatal to both laws,

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